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Introduction

Katrina crashed onto the Gulf Coast in late August, 2005, a little more than nine years ago as these pages appear in print. What happened in the grim days and weeks and years that followed is well known by now. No matter how one measures such things, Katrina is very likely to rank as the most *telling* disaster event ever to visit our land, not simply because of the number of persons it killed or the amount of damage it did, but because it has revealed so much about ourselves as a people, the workings of our society, and the times we live in. Katrina has been the object of a good deal of social science scrutiny for all those reasons. And not only in this country. A collection of essays is being gathered in far-off Australia, for example, on the “global impact” of Katrina: “Perhaps no other disaster of the twenty-first century has so captured the global media’s attention,” the editors’ note. So there is a good deal more to come, a continuing effort to understand what has to be viewed as a continuing disaster.

Our object in this section is to focus on a program of research that has been underway since shortly after the appearance of Katrina. We will be dealing here not with the *findings* of that research so much as the *form* it began with and the *course* it took. The Katrina Task Force, as we came to be called, approached its investigations in ways that turned out to be somewhat atypical of sociological research in general, and that is to be among our topics here.

What follows consists of three longer presentations and three brief commentaries. I will open discussion because I was initially given the responsibility of gathering the Task Force and of securing funds to support its research activities. My subject, then, will be the origins and the early development of the program. The second presentation is by four social scientists from a group of twelve who came together to merge their separate research itineraries, most of them already underway, into a collaborative inquiry and then into a collaborative book. Steve Kroll-Smith and Rachel Madsen, finally, will turn to some of the lessons we think we learned from our work in general, concentrating less on the results of our research than on the paths we took to reach them.

Our efforts, especially in the early years, were assisted by a remarkable Advisory Board, its members looking in on what we were doing with the wisdom of experienced outsiders and the sensitivity of knowing insiders all at once. You will hear more about them shortly. Two of the closest of them, Harvey Molotch and Carol Stack, have agreed to offer comments, and they are joined by Shirley Laska, who, as you will see in a moment, has resided from the beginning at the very heart of our enterprise.

I

As is so often the case with narratives, this one has to focus first on the story teller. But the pronouns that dominate the early going here—"me" and "I"—will soon yield to "we" and "us." That can be read as advance notice of the moral to come.

The story opens at a meeting in New York in early September, 2005. In attendance were Cynthia Fuchs Epstein, then President of the American Sociological Association; Troy Duster, the immediate Past President; and Frances Fox Piven, the President-Elect. Sitting at the same conference table—indeed, providing the table itself as well as the office that contained it—was Craig Calhoun, then President of the Social Science Research Council. Quite a conclave, that. They invited me to spearhead some kind of research initiative on Hurricane Katrina, which had landed but a few days earlier. I had been in and around the offices of the ASA over a number of years and knew something of the recent history of the Association, and I could not recall any occasion in recent times when its senior officers had taken on the task of encouraging research into what was at that point a singular event. I was really impressed. That topic is not worth going into now, but it is certainly worth remembering, and applauding.

I agreed to the proposal, as we all knew I would. I was retired by then and in a position to make the required investment of time. But I then said something like: "You invited exactly the right person to do this, but you don't know why. The core of the group of investigators you want me to assemble already exists—sociologists who have the right kind of intellectual tuning, the right set of skills, the right amount of experience and seasoning. And, moreover, they are already in place."

What prompted that comment will take me to my first story within a story. Once upon a time in the early 1990s, a meeting took place in Lafayette, Louisiana. A few of us disaster specialists had spent most of the afternoon in a symposium speaking about what we knew best: torment, sorrow, devastation, horror, neglect. Afterward, we retired to the nearest bar. A person who some of us knew came over to our table with a warm smile but with a trace of exasperation in his voice and said: "You people devote your whole lives to studying misery and

you just spent three hours talking about it, and yet here you are, laughing and having a good time!” And then he left. That observation really struck a chord with us, and it started a new conversation around the table. One of us, Bill Freudenburg, said something like: “Alright, then, let’s figure out how to make the most of it.” And before long, we had put together an informal gathering that met frequently over the next decade and a half. Our intellectual center of gravity, of course, was the study of disaster and risk. But our geographic center of gravity became New Orleans because so many participants lived there or nearby and so many others had reason to visit frequently. I will not even begin to list the key members of that collectivity—the active roster changed over time in any case—but I will mention a few who figure later in this account: Vern Baxter, Lee Clarke, Bill Freudenburg, Bob Gramling, Pam Jenkins, Steve Picou, myself, and, serving as the group’s central nervous system, Steve Kroll-Smith, and Shirley Laska.

So, getting back to the main narrative, it was a relatively easy matter to bring the core of a research group together. Several of us gathered within two or three weeks of Katrina. It will date the occasion for some if I note that it took place before Hurricane Rita followed so closely on her cruel sister’s heels. There were no rooms in any inn—or hotel, motel, lodge, hostel, bed and breakfast, trailer camp—within a hundred miles of New Orleans. So we met in the home of Bob Gramling in Lafayette. There were air mattresses scattered all about. The map of the household back then was: sleeping under the kitchen table, Steve Kroll-Smith; sleeping under the dining room table, Bill Freudenburg; sleeping on the living room floor, Steve Picou; and located in other niches throughout the homestead were Shirley Laska, Bob Gramling, and me. We all have stories to tell about how we managed to get into and out of Lafayette, but that has to be another matter for another time.

Among the decisions we made at that early meeting was to shift our center of operations to the Social Science Research Council, which was, in essence, a decision to commit ourselves over the long haul. The ASA had really started something.

II

From that moment on, the story spreads out into lines of expansion like the branches of a tree.

Of Meetings: Our evolving group met a few weeks later in Mobile, and then, as soon as fate would permit it, there were three gatherings in New Orleans. From then on, we drifted with winds of opportunity—taking advantage of national and regional conventions, for example, to which members of the Task Force already had their expenses covered. Those winds took us to

Greensboro, Montreal, Boston, New York, San Francisco, and San Antonio, as well as several further gatherings in New Orleans.

Of Grants: Funds came to us in a fairly steady spiral upward, one of the more welcome chapters of the story. Smaller investments appear to have encouraged larger ones. We began with \$8,000 from the ASA (thus the air mattresses in Lafayette and the conference in Mobile). Then Russell Sage awarded us \$35,000 and MacArthur \$260,000 with the intention of helping plant seeds into the ground. With that support in hand, requests to support particular research activities followed, resulting in \$215,000 from Ford, \$300,000 from Rockefeller, and, finally, \$1,400,000 from Gates. I do not want to push the theme of “communality” and “collectivity” too soon here, but I do want to note that those earlier grants had as their object to attract other sources of support, to invite further participation, to create motion. That’s what “seed” means. And that final gift from Gates, rounding out our original goal, was in large part a *recognition* of that earlier activity. So in one sense, at least, “we” became an appropriate pronoun even then. Gates was confirming ongoing research efforts as well as making new ones possible. A more welcome form of momentum is hard to imagine.

Of Growth: The original research group expanded in increments small and large as we learned of scholars who were engaged in the same kind of work we were, or as they learned of us. You will be hearing about them when we turn to the developing research projects shortly. And so with the Advisory Board. Some became advisors to projects already underway and often joined group meetings as participants and contributors—becoming that combination of insider and outsider I referred to before: Bonnie Thornton Dill, Nancy Foner, Heidi Hartmann, Harvey Molotch, Ruben Rumbaut, Kathleen Tierney, and Mary Waters. Some served on an informal executive committee to the Board, meeting for the most part with Craig Calhoun and me in the offices of the SSRC: Susan Cutter, John Darley, Ira Katznelson, Nicholas Lemann, Katherine Newman, and Kenneth Prewitt. And, finally, some participated actively in meetings with the Task Force itself, about which more later.

III

In the next few minutes, I propose to provide a brief outline of the program of research we sought funding for. Steve Kroll-Smith and Rachel Madsen will deal with the publications that came out of the other end of the pipe line. This begins the story. The moral comes later.

First, more than one million persons were displaced in the time of Katrina. Hundreds of thousands of them had not found a way to return home at the time we sought funds (and many of them have not managed to do so yet). They were forming a true diaspora, and we thought it very important to find out what was happening to them. We had every reason to suppose that they were as vul-

nerable a population group as could easily be imagined, since they had lived precarious lives before the rude appearance of Katrina, were the population sector hit hardest by it, had been in exile from their homes in the time since, and were on their way to becoming permanent migrants. That is like suffering three concussions in a row. What was known about them was sketchy at best, but the available findings, most of them anecdotal, were grim. Two of our projects were forays out into that vast, uncharted land, as you will see—one of them the subject of the following article. But several other projects you will be learning of have long sections devoted to the experience of being displaced on the part of persons who *were* able to return home in time to serve as respondents in other of our studies. Some of those returnees had been displaced for months and even years.

Second, life has been a trial for vast numbers of persons who have tried to restore some semblance of the life they knew before the disaster, and the problem for them has been not only what is going on in their heads and hearts and households, but what is going on in their local communities. One of our projected studies was to be an ethnographic profile of two devastated neighborhoods in New Orleans itself, and the other an ethnographic profile of three devastated small communities along the Louisiana coast.

Third, most of our studies are designed as deep probes into portions of the larger social landscape. Add them together, as we plan to, and the sum of them amounts to wide coverage. But it is also important to develop a broader overview of that landscape so as to locate those probes in time and place. One of our most important research efforts, then, was a longitudinal study of people hit hardest by Katrina across the territory it entered in full force. This study is based on telephone interviews of a random sample of 2000 individuals located in the three most badly damaged counties in Mississippi and three most badly damaged parishes in Louisiana. Far more expensive surveys have been undertaken elsewhere, but they share the disadvantage that their samples were drawn from lists supplied by the Red Cross or the Federal Emergency Management Agency, meaning that they were made up of individuals who had applied for this or that benefit in the time of trouble. In that sense, the lists consisted entirely of persons who had selected themselves for inclusion. Moreover, those lists became ever less helpful as time passed because so many of the persons who appeared on them initially were later lost from sight in the driftings from place to place that became the fate of so many of them. No study of this breadth has been undertaken so far, and none, so far as we know, is contemplated. It will be unique.

Fourth, three of the studies included in our research program covered essentially the same ground as others already described here but focused on particular population groups gathered there. One of them dealt with “the chil-

dren of Katrina,” large numbers of whom had resettled in New Orleans with their families after a considerable time away, and another focused on “the women of Katrina,” most of them evacuees with families in tow. The third study is of a family of over 100 closely connected persons with roots deep in the cultural soil of Louisiana. That remarkable group of people can be called an “extended family,” of course—with an emphasis on the “extended,” but terms like “community” and “network” and even “village” are not that far from the mark.

Fifth, two projects in our program reach out beyond the people and persons caught up in the immediate disaster to see Katrina in a broader context. The first is over a comparison of what has to be the two most iconic disasters in American history, both of them involving important sea ports, and separated in time by almost an exact century—the San Francisco earthquake of 1906 and the New Orleans catastrophe of 2005. The second, a welcome newcomer to the list, notes that the story of Katrina has worked its way deep into the American grain, asks what that event and its aftermaths have contributed to American sense of identity, and wonders whether that has become yet another source of trauma on a national scale.

Sixth, one theme that has worked its way through most of the research undertaken by the Task Force is captured in the heading of a conference that took place in New York in late 2011 chaired by Craig Calhoun and me: *What Katrina Can Tell Us About Race, Class, and Gender in These United States*. A number of persons from the Task Force were there to speak of their own work and to hear presentations from members of the Advisory Board: Elijah Anderson, Patricia Hill Collins, Mindy Thompson Fullilove, Douglas Massey, Cecilia Menjivar, and Mary Waters, and with Andrew Beveridge, Cynthia Fuchs Epstein, Heidi Hartmann, Ira Katznelson, Frances Fox Piven, Bill Quigley, Carol Stack, and William Julius Wilson joining in. We hoped that a publication would emerge from the meeting. That did not happen, but the subject itself—race and class in these United States—was touched on in all of the projects we undertook.

One final note, this on a study that was published elsewhere but was nonetheless a project of the original Task Force: anyone who looks carefully at what happened in New Orleans and along the Gulf Coast when Katrina struck will soon be drawn backward into a consideration of the past, where the terrain that Katrina selected as its landing place had been so reshaped by human hands that it had become one of the most vulnerable landscapes to be found anywhere. We call the storm that came over the horizon “Katrina,” and we give that name to the terrifying things that happened afterward, but the real disaster may have been what we humans did to the land long before Hurricane Katrina began to gather force out in the Atlantic.

The other studies I have been sketching so briefly here will be published as a set by the University of Texas Press to be called *The Katrina Bookshelf*. The first volume is now in print, and is the subject of the next article. Kroll-Smith and Madsen will write about the others when their turn comes. This would be as good a time as any for me to add that the original plan called for the *Katrina Bookshelf* to open with a brief introductory volume by Lori Peek and me. That project drifted so far back in the order of things that we will now try to make the most of that and describe it as a closing volume.

I think it is fair to assert that this body of research, together with other studies completed elsewhere, will be among the most comprehensive social science coverage of any catastrophe—whatever your definition—to be found in our literature. One could go a step further, in fact, and suggest that it is one of the most comprehensive coverages of any historical *event* in our literature. Whatever claim one makes for it, the Katrina Bookshelf is something distinctly different.

We hope that the set will serve as a kind of template for the study of disasters, which our world is almost sure to see more of as we all move into the future. The Shelf is not meant as a guide to how research should be undertaken on that dark subject so much as a set of questions that might be raised, approaches that might be weighed, considerations that might be taken into account, and so on.

I will bring this discussion of the Katrina Bookshelf to a close with the suggestion that it is the *set*—the very *fact* of the set—that says the most about the nature of our research program (and, I will add, the vision of the Social Science Research Council and the University of Texas Press). This enterprise has been profoundly collaborative from that very first day in Lafayette. That is the moral I have been trying to get up a sufficient head of steam to turn to.

IV

Our project was well underway—the third or fourth meeting, perhaps—when we looked across the table at one another and realized that none of us could remember being part of any research effort quite like this one. We spoke of it briefly and then went on about our business. A year or so later, Harvey Molotch joined us at a meeting in New Orleans. He was new to our gatherings but a long-time colleague of many of us and a very good friend of the project. He described what we were engaged in as “a new way of doing sociology,” and spoke of the need to write something about it. I do not think any of us had thought of it in quite that way, but the remark had a lasting effect on me and a number of others. In some ways, the forum you are a part of now is a product of that moment.

We sociologists often call upon the term “community” to speak of things for which there are no other words, things that have their meaning in what is not said. If I were to suggest that our research program took on a form that could not be characterized by adding up the separate contributions to it, I would run the risk of sounding like Emile Durkheim at his most enigmatic. But, in a sense, that really is what I mean. The research effort became something larger than the sum of its component parts.

Some of that came from the sense of being caught up in that monstrous creature, Katrina. The storm had a character and a personality as well as a name, and the human sorrow it left in its path made terms like “research” and “project” and “method” seem rather pale. One of our group lived in a house that any sensible engineer might have diagnosed as beyond repair after the floods of Katrina had done their work, but it was brought back to life—given breath—by neighbors in face masks and what almost looked like combat gear who would simply not let it die. Another lived in a home with a huge tree collapsed across its roof, sitting in the middle of acres of dark, empty houses. A scene like that can give new meaning to the word “desolation.” And there were illnesses within our ranks, too, that would have been hard to account for were it not for Katrina. Moreover, being with persons in the neighborhoods the team visited, suffering in such obvious ways, made it hard to think in terms of “respondent” or “subject” or members of “a sample.”

Those reflections may sound a bit more like theater than social science. Our work relies on a stern level of detachment and distance and cool calculation, and we managed to make those translations when necessary. But such reflections help identify a species of collegueship and—again, for lack of a better word, community—from which an important kind of social research can emerge.

V

One more story, almost a closing parable, within a story. Early in the life of the program, I placed a notice in the ASA Newsletter asking readers who were engaged in local projects on Katrina to let me know about their research. We heard from a remarkable list of persons, most of whom were working alone and taking advantage of unusual opportunities found in their own community backyards. I already knew of the work of Jacquelyn Litt, who was interviewing evacuee women, almost all of them with children and families to take care of, who had settled for the moment in Columbia, Missouri, the place where she then lived. Before long, I had also heard from

Beverly Mason, herself a victim of Katrina, who was interviewing evacuees in a trailer camp in Baker, Louisiana;

Lee Miller, interested in learning how civic leaders in her own home town of Huntsville, Texas, were reacting to the appearance of waves of evacuees; Jessica Pardee, another victim of the flooding of New Orleans, who was interviewing the mothers of families who were residents of a housing development; and eight others, all of them from different parts of the country doing research on persons who had been displaced by Katrina.

Litt and I conferred about this rare windfall and decided to invite them all to a gathering in New Orleans to compare notes on the work they were doing. I do not have the time now to go into the human chemistry of what followed, and am not sure that I could find the words to do so in any case. But before long a remarkable collective had taken form that met ten or twelve times in the years to follow and were in constant touch with each other during the stretches in between. There were twelve of them, and they produced the book I mentioned earlier. To have that many colleagues contribute to an anthology is no great thing, of course, but in this instance every one of the twelve both read and commented on every other chapter in the set. That involved endless hours of conversation and endless flows of e-mails, as the parts slowly shaped the whole.

When a merging of different projects like that takes place, it becomes ever more likely that investigators will come to address similar subjects and ask similar questions of the people they are studying, and at that point those scattered groups gradually coalesce into a population about which we know a good deal. The rules do not permit us to speak of this as a “sample,” random or otherwise, but it is not unreasonable to suggest that we have a very large *N* here and that no other interview study of displaced people we are aware of can match it.

Two things more by way of conclusion.

First, the collaborative was made up of both seasoned veterans of the professional world and relative newcomers to it, some of them barely into the dissertation stage. But I will propose that no one, listening in on those proceedings, would be able to tell who was the elder and who the acolyte. If you were to listen to the *content* of the remarks being made, of course, you would be able to make out who was speaking from longer experience and who was newer to that kind of activity. But if you were to listen to the *tone*, you would not hear the familiar sounds of status—the accents of authority, the inflections of hierarchy. Rank, like all weapons, was checked at the door.

Second, it may be important to add that the reward system in use throughout the rest of our professional world is not affected in any obvious way by this kind of collaborative research. Everyone in the group writes under her own name, so one’s professional standing rises and falls in the same tides as every-

one else's. But I think every one of the authors would now agree that each of their reports reflects a collective perspective and a collective wisdom that can only result from group process.

(I should stop and note here that if this article had footnotes, I would insert one now. I used the pronoun "her" in the paragraph above for good reason. I do not think any of us fully realized until we sat down at the table that every one of the twelve investigators was a woman. I do not have the courage to speculate on the reasons, outside of citing the rules of chance. But I will say as I bring this story within a story to a close that if I were invited to bring together into one meeting the dozen most intellectually generous, most empathetic, and least self-absorbed men of my own generation, I doubt very much that we would have seen anything like the same outcome. Am I going to take that thought anywhere? No. Am I going to do anything more with it? No. That's the beauty of footnotes.)

On, now, to the collaborative:

ENDNOTE

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